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# Maimonides Rule Redux

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## Abstract

We use Maimonides Rule as an instrument for class size in large Israeli samples from 2002-2011. In contrast with Angrist and Lavy (1999), newer estimates show no evidence of class size effects. The new data also reveal enrollment manipulation near Maimonides cutoffs. A modified rule that uses birthdays to impute enrollment circumvents manipulation while still generating precisely estimated zeros. In both old and new data, Maimonides Rule is unrelated to socioeconomic characteristics conditional on a few controls. Enrollment manipulation therefore appears to be innocuous. We briefly discuss possible explanations for the change in class size effects since the early 1990s.

The Maimonides Rule research design for estimation of class size effects exploits statutory limits on class size as a source of quasi-experimental variation. As first noted by Angrist and Lavy (1999), Israeli schools face a maximum class size of 40, so that, in principle, grade cohorts of 41 are split into two classes, while slightly smaller cohorts of 39 may be taught in one large class. This produces a distinctive sawtooth pattern in average class size as a function of grade-level enrollment, a pattern seen in Israeli data on enrollment and class size as well in data from school districts around the world.

Analyzing data on class average scores for the population of Israeli 4th and 5th graders tested in June 1991, Angrist and Lavy (1999) reported a substantial return to class size reductions – on the order of that found in a randomized evaluation of class size for US elementary grades (discussed by Krueger 1999). Many applications of the Maimonides Rule research design in other settings also report statistically significant learning gains in smaller classes (see, e.g, the Urquiola 2006 results for Bolivia). Other studies exploiting Maimonides Rule, however, find little evidence of achievement gains from rule-induced class size reductions (as in the Angrist, Battistin and Vuri 2017 study of Italian schools).

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This paper revisits the class size question for Israel with more recent data and a larger sample than that used in Angrist and Lavy (1999). Specifically, we look at a large sample of Israeli 5th graders tested between the school years ending spring 2002 and spring 2011. This update uncovers two findings. First, an econometric analysis paralleling that in Angrist and Lavy (1999) generates robust, precisely estimated zeros. Second, the new data reveal enrollment manipulation at Maimonides cutoffs: there are too many schools with enrollment values that produce an additional class.

Our investigation of enrollment patterns suggests a simple explanation for enrollment manipulation, and allows a straightforward remedy. A memo from Israeli Ministry of Education (MOE) officials to school leaders cautions headmasters against attempts to increase staffing ratios through enrollment manipulation. In particular, schools are warned not to move students between grades or to enroll those overseas so as to produce an additional class. This reflects MOE concerns that school staff adjust enrollment (or enrollment statistics) close to cutoffs so as to produce smaller classes (e.g., by driving enrollment from 40 to 41, and thereby opening a second class). School leaders might care to do this because educators and parents prefer smaller classes. MOE rules that set school budgets as an increasing function of the number of classes also reward manipulation.

We address this problem by constructing an alternative version of Maimonides Rule that is largely unaffected by manipulation. The alternative rule pools students in 4th-6th grade and uses information on their birthdays to impute enrollment by applying the official birthday cutoff for 5th grade enrollment to a sample that includes all students in 4th-6th grade with birth dates that make them eligible for 5th grade. Imputed enrollment also generates a strong first stage for class size, but shows no evidence of sorting around birthday-based Maimonides cutoffs. Moreover, class size effects estimated using the statutory rule are also small, precisely estimated, and not significantly different from zero. Consistent with the absence of manipulation, Maimonides Rule constructed from imputed enrollment is unrelated to socioeconomic status.

Finally, we return to the 1991 data analyzed by Angrist and Lavy (1999). As first noted by Otsu, Xu and Matsushita (2013), these data show evidence of sorting around the first Maimonides cutoff.<sup>1</sup> As in the more recent data, however, enrollment sorting in the original Maimonides sample

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<sup>1</sup>Figure 2 in Otsu, Xu and Matsushita (2013) appears to exaggerate this; we discuss corrected estimates of the 1991 sorting pattern below.

does not appear to be highly consequential for class size effects. In particular, we show that the original formulation of the rule (constructed using November enrollment) is unrelated to students' socioeconomic status. More recent data show small correlations between Maimonides Rule and socioeconomic status, but these disappear when estimated with a few school-level controls.

The birthday-based imputation used to eliminate enrollment sorting in recent data cannot be applied in the older data because birthdays and individual test scores are unavailable for the earlier period. But other simple corrections, such as a “donut” estimation strategy that discards observations near the first cutoff, leave the original results substantively unchanged.<sup>2</sup> The discrepancy between the old and new class size effects therefore seems more likely to be due to a change in the Israeli education production function rather than a sorting artifact. As we discuss in the conclusion, in light of the 2002-2011 results, the evidence for a large, externally valid class size effect in Angrist and Lavy (1999) also seems weaker in hindsight. It now seems especially noteworthy that estimates for a 1992 sample of 3rd graders reported in Angrist and Lavy (1999) show no evidence of achievement gains in smaller classes. Use of a more modern cluster adjustment in place of the parametric Moulton correction used in the original Maimonides Rule study also increases the uncertainty associated with the original estimates.

The next section reviews institutional background on the Israeli school system. We then document the Maimonides first stage in our more recent sample, explain how our birthday-based Maimonides instrument is constructed, and show that birthday-based imputed enrollment generates no evidence of running variable manipulation. Section 4 reports two-stage least squares (2SLS) estimates constructed using the two alternative Maimonides' instruments, and Section 5 looks again at the 1991 and 1992 samples. The conclusion considers possible explanations for changing class size effects.

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<sup>2</sup>Barreca et al. (2011) appears to be the first to use the donut strategy to examine the consequences of sorting near regression discontinuity cutoffs.

# **I. Background and Context**

## **A. Israeli Schools**

Schooling in Israel is compulsory beginning in first grade, starting around age 6. Israeli students attend neighborhood schools, which serve catchment areas determined by a student's home address. Our analysis focuses on secular and religious students in Jewish public schools, the group that constitutes the bulk of public school enrollment. Public schools are administered by local authorities, but funded centrally by the MOE. Maimonides Rule, which caps class sizes at 40, has guided class assignment and school budgeting since 1969. The rule is well-known among school administrators and teachers. Most parents have few options by way of school choice other than to move. We therefore expect any manipulation of enrollment to reflect the behavior of teachers and school administrators rather than parents.

## **B. Related Work**

Maimonides-style empirical strategies have been used to identify class size effects in many countries, including the US (Hoxby 2000), France (Piketty 2004 and Gary-Bobo and Mahjoub 2013), Norway (Bonesronning 2003 and Leuven, Oosterbeek and Ronning 2008), Bolivia (Urquiola 2006), and the Netherlands (Dobbelsteen, Levin and Oosterbeek 2002). On balance, these results point to modest returns to class size reductions, though mostly smaller than those reported by Angrist and Lavy (1999) for Israel. A natural explanation for this difference in findings is the large size of Israeli elementary school classes. In line with this view, Woessmann (2005) finds a weak association between class size and achievement in a cross-country panel covering Western European school systems in which classes tend to be small. Recently published regression estimates for Israeli using 2006 and 2009 data show no evidence of a class size effect; this study also documents the vigorous debate over class size in Israel (Shafrir, Shavit and Blank 2016).<sup>3</sup>

A number of studies look at data manipulation and how this might compromise attempts to estimate causal class size effects. Urquiola and Verhoogen (2009) uncover evidence of sorting around Maimonides cutoffs in a sample from Chilean private schools. Angrist, Battistin and Vuri

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<sup>3</sup>Results in Sims (2008) suggest class size reductions obtained through combination classes have a negative effect on students' achievement.

(2017) show that estimates from Maimonides style experiments in southern Italy probably reflect increased manipulation of test scores by teachers in small classes. As noted above, Otsu, Xu, and Matsushita (2013) report evidence of sorting around the first Maimonides cutoff in the Angrist and Lavy (1999) sample; we return to this finding below. In related work, Jacob and Levitt (2003) document manipulation of test scores in Chicago public schools.

Methodological investigations of sorting in regression discontinuity (RD) running variables originate with McCrary (2008), who introduced the statistical test for running variable manipulation used here. Barreca, Lindo and Waddell (2016) show that manipulation and nonrandom heaping of a running variable can bias RD estimates. Barreca et al. (2011) explore manipulation of the birthweight data used by Almond et al. (2010) to identify the causal effects of neonatal health care. Gerard, Rokkanen and Rothe (2018) derive bounds on causal effects estimated using RD designs that are built on running variables compromised by sorting. Arai et al. (2018) introduce a test for validity of fuzzy regression discontinuity designs based on the joint distributions of treatment status and observed outcomes at cutoffs, applying this to the Angrist and Lavy (1999) sample. This test suggests manipulation of 1991 enrollment is not a source of bias in the original Angrist and Lavy estimates.

## **II. Data and First Stage**

### **A. Data and Descriptive Statistics**

The test scores used in this study come from a national testing program known as Growth and Effectiveness Measures for Schools, or GEMS. Starting in 2002, fifth graders in half of Israeli schools were sampled for participation in GEMS (which also tests 8th graders). Tests are given in math, native language skills (Hebrew or Arabic), science and English. GEMS test scores are reported on a 0-100 scale, similar to the scale used in Angrist and Lavy 1999. Math scores average around 68, with standard deviation of about 11 for class average scores; language scores average around 72, with a standard deviation around 8 for class averages. Student-level standard deviations are roughly double the standard deviations of class means. These statistics appear in Appendix Table A1. The appendix also describes the GEMS data further.

Data on test scores were matched to administrative information describing schools, classes, and students. The unit of observation for most of our statistical analyses is the student. School records include information on the enrollment figures reported by headmasters to the MOE each November. This enrollment variable, henceforth called “November enrollment”, is used by the MOE to determine school budgets. We also have data on class size collected at the end of the school year, in June. We refer to this variable as “June class size”. Individual student characteristics in the file include gender, parents’ education, number of siblings, and ethnicity. Schools in the GEMS samples are identified as secular or religious. Each school is also associated with an index of socioeconomic status (SES index).<sup>4</sup>

Our statistical analysis looks at fifth grade pupils in the Jewish public school system, including both secular and religious schools. The analysis excludes students in the special education system, who do not take GEMS tests. Our analysis covers data from 2002 through 2011 (2002 was the first year of the GEMS tests). In 2012, the MOE began implementing a national plan to reduce class size, rendering Maimonides’ Rule less relevant (Vurgan 2011). We focus here on math and (Hebrew) language exam results.

The matched analysis file includes 240,310 fifth grade students from 8,823 classes. The data structure is a repeated cross-section; the sample of GEMS schools changes from year to year. Table A1, which reports descriptive statistics for classes, students, and schools in the estimation sample, shows that the mean and median elementary school class has about 28 pupils, and there are roughly 58 pupils and 2 classes per grade. Ten percent of classes have more than 35 pupils, and 10 percent have fewer than 21 pupils. Demographic data show that 90 percent of students are Israeli-born. Many in the sample are the children of immigrants; 16 percent are the children of immigrants from the former Soviet Union, for example.

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<sup>4</sup>The school SES index is an average of the index for its students. Student SES is a weighted average of values assigned to parents’ schooling and income, economic status, immigrant status and former nationality, and the school’s location (urban or peripheral). The index ranges from 1-10, with 1 representing the highest socioeconomic level. Schools with more disadvantaged students (high SES index) receive more funding per student. We observe only the school average SES.

## B. The Maimonides First Stage

Maimonides' Rule reflects MOE regulations requiring that classes be split when they reach the statutory maximum of 40. Strict application of the rule produces class sizes that are a non-linear and discontinuous function of enrollment. Writing  $f_{jt}$  for the predicted 5th grade class size at school  $j$  in year  $t$ , we can write rule-based enrollment as

$$f_{jt} = \frac{r_{jt}}{\lceil \text{int}((r_{jt} - 1)/40) + 1 \rceil}, \quad (1)$$

where  $r_{jt}$  is the November enrollment of 5th graders at school  $j$  in year  $t$ , and  $\text{int}(x)$  is the largest integer less than or equal to  $x$ .

Appendix Figure A1 plots actual average June class size and rule-based predictions,  $f_{jt}$ , against November enrollment. Plotted points show the average June class size at each level of enrollment. The fit here looks similar to that reported using 1991 data in Angrist and Lavy (1999). Predicted discontinuities in the class size/enrollment relationship are also diminished by the fact that many classes are split before reaching the theoretical maximum of 40.

The first-stage effect of  $f_{jt}$  on class size is estimated by fitting

$$s_{ijt} = \pi f_{jt} + \rho_1 r_{jt} + \delta_1' X_{ijt} + \gamma_t + \varepsilon_{ijt} \quad (2)$$

where  $s_{ijt}$  is the June class size experienced by student  $i$  enrolled in school  $j$  and year  $t$ ;  $X_{ijt}$  is a time-varying vector of student and school characteristics,  $f_{jt}$  is as defined above, and  $\varepsilon_{ijt}$  is a regression error term. The student characteristics in this model include a gender dummy, both parents' years of schooling, number of siblings, a born-in-Israel indicator and ethnic-origin indicators. School characteristics include an indicator for religious schools, the school SES index, and interactions of the SES index with dummies for the 2002-3 period and the 2008-11 period.<sup>5</sup> We also include year fixed effects ( $\gamma_t$ ) and control for alternative functions of the running variable,  $r_{jt}$ .

Estimates of  $\pi$  in Equation (2) are remarkably stable at around 0.62. This can be seen in Appendix Table A2, which reports first stage estimates using a variety of running variable controls, including linear and quadratic functions of enrollment and the piecewise linear trend used by An-

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<sup>5</sup>Interactions of the SES index with dummies for these two periods control for changes in the weights and the components of the index implemented in 2004 and 2008.



grist and Lavy (1999). This trend function picks up the slope on the linear segments of the rule. Specifically, the trend is defined on the interval  $[0, 200]$  as follows:

$$\begin{array}{ll}
r_{jt} & r_{jt} \in [0, 40] \\
20 + r_{jt}/2 & r_{jt} \in [41, 80] \\
100/3 + r_{jt}/3 & r_{jt} \in [81, 120] \\
130/3 + r_{jt}/4 & r_{jt} \in [121, 160] \\
154/3 + r_{jt}/5 & r_{jt} \in [161, 200]
\end{array}$$

The constants here join the Maimonides linear segments at the cutoffs.

### C. Sorting Out Enrollment Sorting

The budget for Israeli primary schools comes from local municipal authorities and the national MOE. The local authority funds administrative costs, while the MOE funds teaching and other educational activities. The MOE's budget for instruction time is based on the predicted number of classes determined by the November enrollment figures reported to the MOE (Ministry of Education 2015a). This generates an incentive to manipulate enrollment, either directly by moving students between grades, or through false reporting.<sup>6</sup>

As first noted by McCrary (2008), manipulation of a running variable may be revealed by discontinuities in the running variable distribution. Figure 1 plots the histogram of November enrollment in our 2002-11 sample. Vertical lines indicate Maimonides cutoffs. The figure shows a clear spike in enrollment just to the right of the cutoffs at 40 and 80, with apparent holes in the distribution to the left.

The forces producing these spikes are hinted at in MOE memoranda on enrollment reporting distributed at the end of the school year. These memoranda remind headmasters of the need for accurate enrollment reporting to determine funding. The 2015 circular also cautioned headmasters against enrollment manipulation. In particular, schools were warned not to move students between grades, to enroll a student in more than one school, or to enroll students residing overseas so as

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<sup>6</sup>Funding rules for 2004-7 were revised so as to make total enrollment the major funding determinant rather than the number of classes but this reform was never fully implemented. In 2007, the MOE returned to the class-based funding rule (Lavy 2012; Vurgan 2007).

to produce an additional class. In 2016, the MOE began auditing enrollment data in an effort to prevent this type of manipulation, though sanctions are as yet undetermined (Ministry of Education, 2015*b*). Interestingly, Figure 1 offers further evidence of financially-motivated enrollment manipulation in the spike at a class size of 20. While budgetary rules set funding as a function of the number of classes, classes with enrollments below 20 are generally allotted half the regular funding.

Although the incentive for headmasters to push enrollment across Maimonides cutoffs seems clear, the question of whether this produces only misreporting or actual movement between grades is less easily addressed. Real enrollment changes can be accomplished by skipping students a grade ahead or through grade retention. A further likely channel is flexible age at entry for first graders. Although the official start age policy specifies a Chanukah-based birthday cutoff (detailed below), in practice, school headmasters have some discretion as to when children may start school.

Appendix Figure A2 suggests that at least some of the enrollment changes resulting from manipulation are real and persistent, rather than misreported. This figure plots the histogram of the number of 5th graders present for the GEMS tests in our sample. The evidence here is strongest for bunching around the first Maimonides cutoff, with somewhat weaker evidence of missing mass to the left of 80. Missing data for values below the second cutoff might be explained by the fact that roughly 10 percent of students enrolled miss the test.

Our primary concern is the possibility of selection bias resulting from enrollment manipulation. We might expect, for example, that more sophisticated school leaders understand the budgetary value of moving enrollment from just below to just beyond Maimonides cutoffs. And schools led by sophisticated leaders may also enroll higher-SES students, on average, producing a spurious achievement increase at the point where rule-based predicted class size drops.

We mitigate selection bias from enrollment manipulation by constructing a version of Maimonides Rule that uses birthday-based imputed enrollment in place of reported November enrollment. Israel's compulsory attendance laws specify rules for student enrollment in first grade according to whether a child's 6th Hebrew birthday falls before or after the last day of Chanukah. Students born after the last day of Chanukah are too young for first grade and must wait an additional year to start school. Most manipulation appears to result from single-grade retention or advancement relative to birthday-based enrollment, either as a result of delayed or accelerated school

entry or advancement since first grade. Data on a sample of 4th, 5th, and 6th graders therefore includes almost all students who should be in 5th grade and can therefore be used to reconstruct the enrollment values that would be observed in a world where school officials follow official rules.

We apply the Chanukah-based birthday rule to June enrollment data for the sample of all 4th-6th graders in each school in the same year we see that school's 5th graders taking GEMS tests. This produces an imputed enrollment variable for 5th graders that is unlikely to reflect manipulation by school officials. Figure 2, which plots the imputed enrollment histogram, suggests that enrollment imputed in this manner is indeed manipulation-free. The figure shows a reasonably smooth distribution, with no evidence of spikes to the right of Maimonides cutoffs or at 20.

The McCrary (2008)-style density plots in Appendix Figure A3 are also consistent with the view that imputed birthday-based rule eliminates sorting in the November enrollment data. The upper panel of the figure plots empirical and fitted densities for November enrollment, allowing for a discontinuity at the first and the second Maimonides cutoffs. Here, the jumps at 41 and 81 seem clear enough. By contrast, Panel B, which shows the same sort of plot for imputed enrollment, suggests the imputed enrollment distribution is smooth through these cutoffs.<sup>7</sup>

Appendix Table A3 reports estimates of the first stage regression of class size on Maimonides rule when the latter is computed using imputed birthday-based enrollment. These estimates are about half the size of those constructed using November enrollment. As when estimating November data, however, key first stage parameters are estimated precisely and largely insensitive to the nature of the running variable control.<sup>8</sup>

### III. Class Size Effects: 2002-2011

Class size effects are estimated using a two-stage least squares (2SLS) setup that models  $y_{ijt}$ , the GEMS score of student  $i$  enrolled in 5th grade at school  $j$  in year  $t$ , as a function of 5th grade class

<sup>7</sup>These plots use DCdensity (<http://eml.berkeley.edu/~jmccrary/DCdensity/>), which generates a graph of estimated densities with standard error bands, allowing for a single discontinuity, as described in McCrary (2008). Dots in the figure are histograms in an one-unit bin width.

<sup>8</sup>Appendix Figure A4 plots actual average June class size against birthday-based predicted enrollment, comparing the birthday-based first stage to the first stage constructed using actual November enrollment. Consistent with the smaller birthday-based first stage, actual class size follows birthday-based predictions less closely, with smoother size changes at Maimonides cutoffs. The non-linear and non-monotonic relationship between enrollment and class size remains.

size, running variable controls, year effects ( $\mu_t$ ), and additional controls,  $X_{ijt}$ . Second-stage models with a linear running variable control can be written:

$$y_{ijt} = \beta s_{ijt} + \rho_2 r_{jt} + \delta_2 X_{ijt} + \mu_t + \eta_{ijt}, \quad (3)$$

where  $\beta$  is the causal effect of interest and  $\eta_{ijt}$  is the random part of potential achievement. The first stage for 2SLS estimation of equation (3) is equation (2).

2SLS estimates of  $\beta$  in equation (3) suggest class size has no causal effect on achievement. Estimates of effects on language and math scores, reported in columns 2-4 and 6-8 of Table 1, range from -0.03 to 0.03 with standard errors around 0.03 to 0.04, and are not statistically different from 0. These reasonably precise zeros contrast with the Angrist and Lavy (1999) estimates around -0.25. Interestingly, OLS estimates of a version of equation (3), reported in columns 1 and 5 of the table, are also small, though positive (indicating bigger classes improve test scores) and significant for math scores. The large precisely estimated negative SES effect reported in Table 1 implies that a 1 standard deviation increase in the school-wide SES index (lower SES) is associated with about 0.1 standard deviation lower language and math score. In estimates not reported in the table, we also see large ethnicity and parental school coefficients. This suggests that our dependent variables are informative measures of student achievement and bolsters the case for interpreting small insignificant class size effects as true zeroes.

It seems fair to say that the education production function identified by Maimonides Rule in more recent data differs markedly from that estimated using similar specifications for 1991 data. The earlier Angrist and Lavy (1999) results are replicated in Appendix Table A4, with the modification that the replication reports “Stata clustered” standard errors (clustered on school) rather than standard errors clustered using the Moulton formula as in Angrist and Lavy (1999). In contrast with the small effects found for 2002-2011, Maimonides Rule instruments in the 1991 sample, with linear running variable controls, generates an estimated effect of -0.277 for 5th grade language (with a standard error of 0.076) and an estimated effect of -0.231 for 5th grade math (with a standard error of 0.099).<sup>9</sup> Estimates for 4th graders are smaller; only that estimated for language with linear enrollment controls is (marginally) significantly different from zero.

Perhaps the new findings showing zero class size effects in recent data are an artifact of running

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<sup>9</sup>As in Angrist and Lavy (1999), 1991 test scores are measured as a composite percentile, ranging from 0-100, with means around 70 and standard deviations of 8-10.

variable manipulation. This possibility is explored in Table 2, which reports a set of 2SLS estimates paralleling those in Table 1, but computed in this case using version of Maimonides rule derived from birthday-based imputed enrollment. Like the estimates in Table 1, the results in Table 2 show little evidence of achievement gains in smaller classes. In the 2002-2011 data, therefore, the lack of a class size effect appears unrelated to school leaders' efforts to open an additional class by pushing enrollment across Maimonides Rule cutoffs.

We also estimated models where the effect of class size on test scores is interacted with the SES index, thereby allowing for the possibility that class size matters most for disadvantaged students. The instruments in this case are  $f_{jt}$ , and  $f_{jt} * SES_{jt}$ , where  $SES_{jt}$  is the SES index for school  $j$  at year  $t$ . These results likewise show no evidence of class size effects or SES interactions. As can be seen in Appendix Figure A5, estimation of class size effects separately for each year also generates small, mixed positive and negative, and (with one exception), insignificant effects. This weighs against the hypothesis that the absence of a class size effect reflects extensive test preparation in more recent data, since Israeli media reports suggest test preparation efforts have intensified over time.

Gerard, Rokkanen and Rothe (2018) note that sorting around RD cutoffs is innocuous when manipulated units are similar to those unaffected by sorting. To check for possible discontinuities in school characteristics induced by sorting, we regressed the school-by-year SES index (increasing from 1 to 10 as SES declines) on Maimonides rule in a version of equation (2) fit to school-year averages. Panel A of Appendix Table A5, reports these results when Maimonides Rule is constructed from November enrollment data, showing schools with larger predicted class size have somewhat higher SES. For example, the estimates in column 2 suggest that a 10 student increase in predicted class size is associated with a reduced disadvantaged index (that is, higher SES) of about 0.2. This seems like a modest change, amounting to less than one-tenth of a standard deviation of the index. The estimates in columns 4-6 of Table A5 show that this relationship disappears when Maimonides Rule is constructed using birthday-based imputed enrollment.

Although encouraging for the thesis that imputed enrollment data are uncompromised by systematic sorting, the results in Panel A of Table A5 suggest we might worry about non-random enrollment manipulation when working with November enrollment. But Panel B of the table shows that the association between November-based Maimonides Rule and SES disappears in models that

control for a pair of school average covariates (fathers' schooling and family size), while these zeros are still precisely estimated. Moreover, Maimonides Rule computed using imputed enrollment is unrelated to SES with or without additional covariate controls. Since our findings on class size are consistent using both enrollment variables and when estimated with and without covariates, it seems unlikely that non-random sorting across Maimonides cutoffs in the November enrollment data is an important source of bias.<sup>10</sup>

## IV. Earlier Estimates Explored

The evidence of running variable manipulation in 2002-2011 data naturally raises questions about manipulation artifacts in the results reported in Angrist and Lavy (1999). Appendix Figure A6 plots estimated enrollment histograms and densities for the Angrist and Lavy samples of 4th and 5th graders tested in 1991. This figure shows evidence of a gap in the enrollment distribution below the first Maimonides cutoff of 41. The figure also reports estimates of the associated densities, allowing for a discontinuity at 41. Here too, we see evidence of a jump.<sup>11</sup> Appendix Figure A7 presents the enrollment histogram for the sample of 3rd graders tested in 1992; this figure shows a somewhat more modest enrollment jump to the right of the first cutoff.<sup>12</sup>

Otsu, Xu and Matsushita (2013) includes figures similar to our Figure A6. These earlier plots, however, appear to count the 1991 enrollment distribution in terms of classes rather than schools. Because many grade cohorts are indeed split into additional classes at or near 40, the number of classes in schools with enrollments just above 40 jumps with or without sorting. The Otsu, Xu and Matsushita (2013) discontinuity check therefore confounds the density discontinuity induced by sorting with the causal effect of Maimonides Rule on the number of classes. This concern notwithstanding, however, Figure A6 indeed shows evidence of sorting around the first Maimonides cutoff in 1991.

Additional analyses of the older data (not reported here) suggest sorting was less pervasive in 1991 and 1992, with little evidence of manipulation beyond the first Maimonides cutoff. Even

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<sup>10</sup>2SLS estimates of class size effects from models without covariates other than running variable controls are small and positive, marginally or not significantly different from zero.

<sup>11</sup>The discontinuity at 81 (the split from 2 to 3 classes) in the 1991 data is not statistically significant

<sup>12</sup>The discontinuity at 41 in the 1992 data is statistically significant; the discontinuity at 81 is not.

so, in view of the discontinuity in the 1991 enrollment distribution seen in Figure A6, it's worth asking whether enrollment manipulation is likely to be a source of omitted variables bias in the older estimates. Table 3 therefore reports estimates from a regression of school-level SES on Maimonides Rule using 1991 data, similar to the estimates reported in Table A5. As in the more recent data (with covariates), we see little evidence of a relationship between Maimonides Rule and school-level SES. The negative associations estimated for 5th graders are not significantly different from zero, while the sign flips to (insignificant) positive for 4th and 3rd graders.<sup>13</sup>

The individual student data required for a birthday-based imputation of 1991 enrollment are unavailable. We turn therefore to an alternative check on the replicated results that omits observations near the first Maimonides cutoff.<sup>14</sup> The results of this further exploration of the consequences of sorting in 1991 are reported in Appendix Table A6. For example, the estimated class size effect of  $-0.234$  in column 1 of Table A6 was computed using a sample omitting schools with 5th grade enrollments between 39 and 41. This can be compared with the full-sample estimate of  $-0.277$ . Although somewhat less precise, the donut estimates in Table A6 differ little from those for the full sample estimates reported in Table A4.

## V. Summary and Conclusion

The Maimonides Rule identification strategy for class size effects generates precisely estimated zeros in large Israeli samples for 2002-2011. These samples also show clear evidence of enrollment manipulation around Maimonides class size cutoffs, likely reflecting school leaders' desire to open an additional class when enrollment is close to a cutoff. Enrollment imputed using information on grade-eligible birthdates appears unaffected by manipulation, however, and 2SLS estimates derived from imputed enrollment instruments show similarly small class size effects. Maimonides Rule constructed using birthday-based imputed enrollment is also unrelated to a school-level measure of SES.

We find only weak evidence of *systematic* enrollment sorting: more recent data generate small estimated effects of the original Maimonides Rule on socioeconomic status, but these effects dis-

<sup>13</sup>The 1991 SES index is scaled as "percent disadvantaged."

<sup>14</sup>Barecca et al. (2011) appear to be the first to propose this simple adjustment for sorting, sometimes referred to as an RD "donut".

appear after conditioning on a few covariates. The fact that estimated class size effects are similar whether Maimonides Rule is constructed using November or birthday-based enrollment further reinforces our conclusion that the finding of a null class size effect in recent data is not a manipulation artifact. The estimates of zero class size effect in more recent data contrast with the substantial negative class size effects reported by Angrist and Lavy (1999). We also see some evidence of manipulation around the first Maimonides cutoff in the older data analyzed by Angrist and Lavy (1999). But the absence of a relationship between Maimonides' Rule and school average SES, and results from a donut strategy that omits data near the cutoff, suggest these estimates too are unaffected by manipulation near cutoffs. This conclusion is likewise supported by specification test results reported in Arai et al. (2018).

The disappearance of Israeli class size effects may reflect changes in the Israeli education production function. The fact that Israeli class size has fallen from a median of 31 in 1991 to 28 in more recent samples may be relevant. Yet Figure A5, which plots 2SLS estimates by year, shows no evidence of declining effects over the period 2002-2011. It may also be relevant that, since the early 2000's, some schools have hired additional teaching staff, a staffing increase funded mostly by parents in high SES schools (Vurgan, 2014). Weighing against the importance of these changes for class size estimates, our analysis fails to show significant class size/SES interactions or significant effects in earlier years.

We briefly explored changes in other inputs that might explain the absence of class size effects in recent data (these data are from an analysis reported in Blass, Tsur and Zussman 2012). Regressions of total hours of instruction provided by school staff and others on predicted class size show small, marginally significant increases on the order of 0.5 percent for each additional student. We also see small, marginally significant increases in the share of class time going to small group instruction. Per-pupil spending however, falls about 2 percent for each additional student. In future work, we hope to be able to identify causal effects of these additional inputs, and better gauge their interaction with class size in education production.

It seems noteworthy that the 1991 estimates reported in Angrist and Lavy (1999) are strongest for 5th graders, but less impressive for 4th graders, for whom only estimates for language are significantly different from zero, and in only one specification. The original Angrist and Lavy study also reported zero class effects in a 1992 sample of 3rd graders, a result attributed in the



original write-up to extensive test preparation and changes in testing protocols. These forces may be at work in the more recent GEMS data analyzed here as well. Some analysts have suggested schools are increasingly and effectively teaching to GEMS tests (e.g. Klinger, 2009). Here too, however, there's no smoking gun for mediating interactions: our analysis uncovers no changes in class size effects over time that might be linked to changes in test preparation.<sup>15</sup> On balance, it seems fair to say that the 1991 results are unusual in showing strong class size effects, while the null effects reported for 1992 have emerged as more representative of the causal relationship between class size and test scores in Israel.

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<sup>15</sup>In view of the unusually early administration of tests in 2004-6, Appendix Table A7 reports estimates analogous to those in Tables 1 and 2, computed in a sample omitting data from 2004-6. This change in sample leaves the results unchanged.

Table 1: Class Size Effects Estimated Using November Enrollment Instruments (2002-2011)

	Language				Math			
	OLS	2SLS	2SLS	2SLS	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Class size	0.0091 (0.0173)	-0.0220 (0.0314)	-0.0299 (0.0329)	-0.0288 (0.0322)	0.0492 (0.0229)	0.0283 (0.0422)	0.0158 (0.0436)	0.0137 (0.0429)
SES index	-0.4268 (0.0602)	-0.4312 (0.0602)	-0.4281 (0.0603)	-0.4286 (0.0603)	-0.3660 (0.0800)	-0.3688 (0.0801)	-0.3640 (0.0802)	-0.3635 (0.0801)
November enrollment	0.0025 (0.0037)	0.0052 (0.0043)	0.0226 (0.0144)		-0.0008 (0.0048)	0.0010 (0.0058)	0.0285 (0.0183)	
Enrollment squared/100			-0.0103 (0.0081)				-0.0163 (0.0101)	
Piecewise linear trend				0.0147 (0.0094)				0.0100 (0.0128)
<i>N</i>	225,108				226,832			

Notes: This table reports OLS and 2SLS estimates of equation (3) in the text. The endogenous variable is June class size; Maimonides Rule is constructed using November enrollment. Standard errors reported in parentheses are clustered at the school and year level. The dependent variable is a math or language test score. Additional covariates include student characteristics (a gender dummy, parents' years of schooling, number of siblings, a born-in-Israel indicator, and ethnic-origin indicators), year fixed effects, an indicator for religious schools, socioeconomic index and interactions of the socioeconomic index with dummies for 2002-3 period and 2008-11 period. The reported SES coefficient is for 2004-7.

Table 2: Class Size Effects Estimated Using Birthday-based Imputed Enrollment (2002-2011)

	Language				Math			
	OLS	2SLS	2SLS	2SLS	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Class size	0.0070 (0.0172)	0.0012 (0.0608)	-0.0089 (0.0666)	-0.0061 (0.0657)	0.0386 (0.0231)	-0.0366 (0.0814)	-0.0623 (0.0889)	-0.0616 (0.0878)
SES index	-0.4254 (0.0602)	-0.4263 (0.0610)	-0.4246 (0.0609)	-0.4252 (0.0609)	-0.3570 (0.0799)	-0.3680 (0.0809)	-0.3636 (0.0810)	-0.3644 (0.0809)
Birthday-based enrollment	0.0033 (0.0035)	0.0038 (0.0060)	0.0163 (0.0184)		0.0037 (0.0046)	0.0099 (0.0081)	0.0418 (0.0239)	
Enrollment squared/100			-0.0068 (0.0086)				-0.0173 (0.0109)	
Piecewise linear trend				0.0113 (0.0151)				0.0318 (0.0203)
<i>N</i>	225,108				226,832			

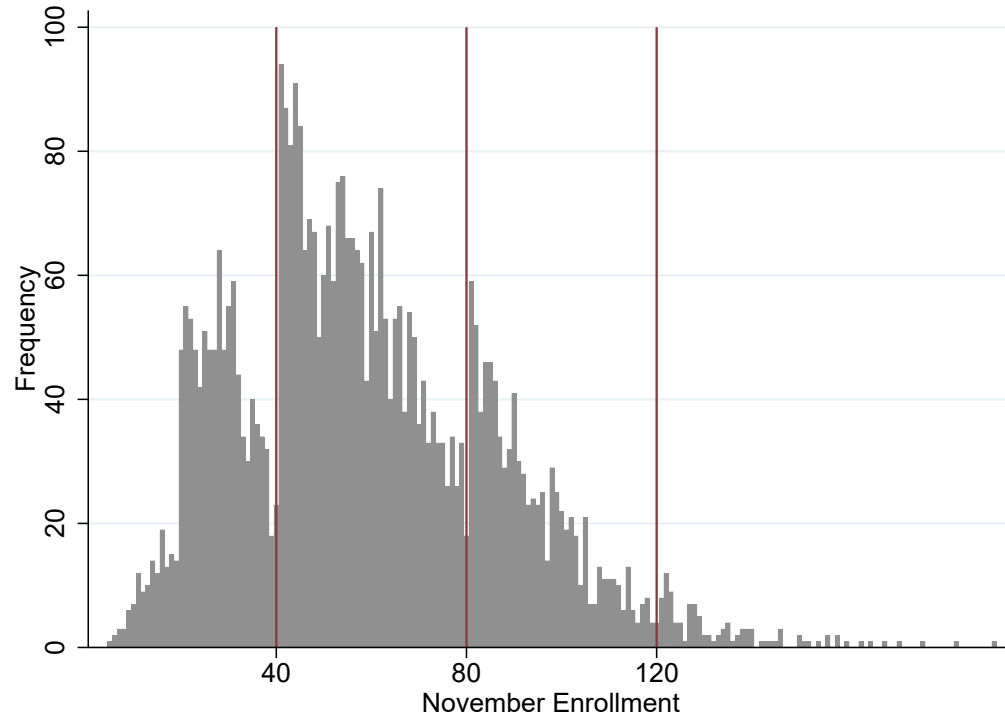
Notes: This table reports OLS and 2SLS estimates of equation (3). The endogenous variable is June class size; Mainmonides Rule is constructed using birthday-based enrollment. Standard errors reported in parentheses are clustered at the school and year level. The dependent variable is a math or language test score. Additional covariates include student characteristics (a gender dummy, parents' years of schooling, number of siblings, a born-in-Israel indicator, and ethnic-origin indicators), year fixed effects, an indicator for religious schools, socioeconomic index and interactions of the socioeconomic index with dummies for 2002-3 period and 2008-11 period. The reported SES coefficient is for 2004-7.

Table 3: Maimonides Rule Effects on Socioeconomic Status in 1991 Data

	Fifth Grade			Fourth Grade			Third Grade		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$f_{jt}$	-0.0592 (0.0784)	-0.0686 (0.0848)	-0.0709 (0.0865)	0.0532 (0.0810)	0.0636 (0.0878)	0.0718 (0.0894)	0.0400 (0.0845)	0.0444 (0.0906)	0.0581 (0.0910)
November Enrollment	-0.0598 (0.0152)	-0.0475 (0.0448)		-0.0691 (0.0155)	-0.0824 (0.0461)		-0.0940 (0.0165)	-0.101 (0.0534)	
Enrollment Squared/100		-0.0068 (0.0235)			0.0076 (0.0248)			0.0041 (0.0307)	
Piecewise Linear Trend			-0.1010 (0.0350)			-0.1332 (0.0349)			-0.1796 (0.0358)
$N$	1,002	1,002	990	1,013	1,013	1,003	989	989	982

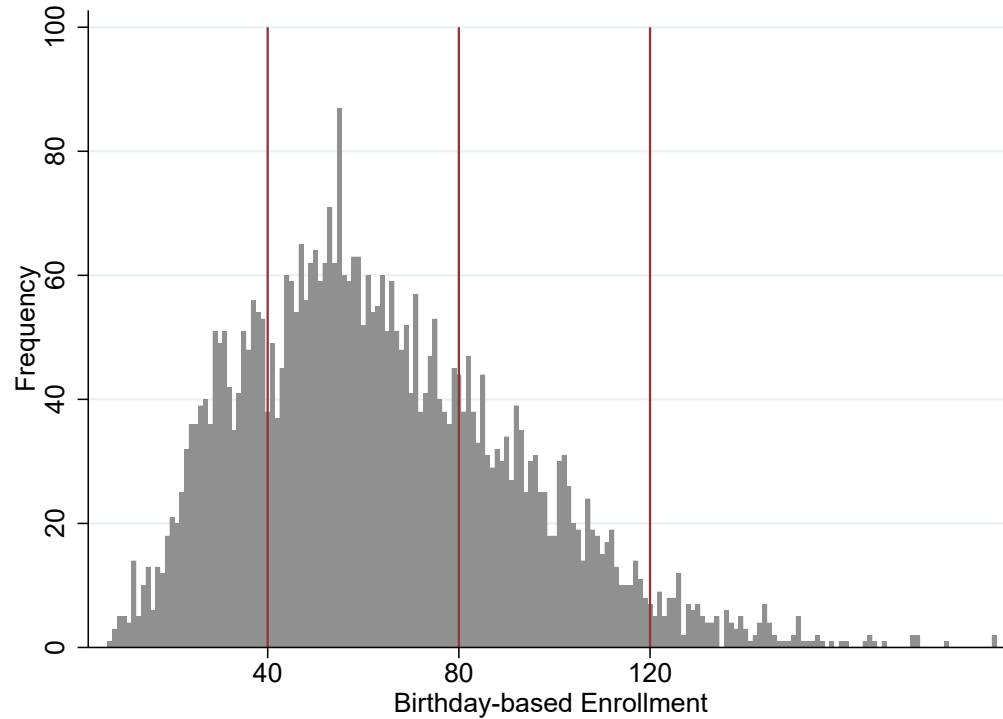
Notes: This table reports OLS estimates of the effect of Maimonides Rule on a school-level index of socioeconomic status. The unit of analysis is the school. The third grade sample is limited to schools appearing in the fourth and fifth grade sample. The piecewise linear control in columns (3), (6), and (9) omits enrollments above 160.

Figure 1: The 5th grade Enrollment Distribution Reported in November (2002-2011)



Notes: This figure plots the distribution of 5th grade enrollment as reported by school headmasters in November. Reference lines indicate Maimonides Rule cutoffs at which an additional class is added.

Figure 2: The 5th Grade Birthday-based Imputed Enrollment Distribution (2002-2011)



Notes: This figure plots the distribution of birthday-based imputed enrollment for 5th graders by school. Birthday-based imputed enrollment is computed from the birthday distribution of students enrolled in 4th-6th grade in June of each year. The birthday rule counts 4th-6th graders born between Chanukah 11 years before and Chanukah 10 years before the current school year. Reference lines indicate Maimonides Rule cutoffs at which an additional class is added.

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# Maimonides Rule Redux

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July 2018

## Online Appendix

### Data Appendix

The data used here are from the 2002–2011 Growth and Effectiveness Measures for Schools (GEMS) testing program. GEMS is administered by the National Authority for Measurement and Evaluation in Education. GEMS scores are used to assess school progress. Individual GEMS scores are not released to students or schools administrators.<sup>1</sup>

GEMS tests are usually given some time between mid-March and mid-June (with the exception of the 2004-2006 school years, when the tests were given in October-November). The GEMS test-takers are drawn from a representative 1-in-2 sample of all elementary and middle schools in Israel, so that each school participates in GEMS once every two years. GEMS tests fifth-graders (primary school) and eighth-graders (middle school) in math, science, native language skills, and English. In principle, all students except those in special education classes are tested; in practice, the proportion of students tested is above 90 percent.

We focus on math and language tests given in Jewish elementary schools, as in Angrist and Lavy (1999). GEMS scores are reported on a 1-to-100 scale similar to that used for the scores in the the original Angrist and Lavy study. Between 2002 and 2006, participating schools were tested in four subjects. Since 2007, only two subjects at a time are tested, either math and language or science and English. Our ten year sample includes the math and language scores of 240,310 fifth graders. Between 2002-2006, this sample covers an annual average of 563 Jewish public (secular and religious) schools and 1162 classes per year. Between 2007-2001, the sample covers an annual average of 286 Jewish public schools and 602 classes per year.

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<sup>1</sup>The Division of Evaluation and Measurement website, <http://cms.education.gov.il/EducationCMS/Units/Rama/Meitzav/>, provides additional background.

In our sample, the average attrition rate is 5 percent. Estimates are similar when the sample is limited to classes in which at least 50 percent of students were tested. Attrition is unrelated to Maimonides Rule. For 51 schools tested in consecutive years, we use the first test only.

We linked the GEMS data to MOE administrative records covering all Israeli 5th graders. Student records include gender, parents' education, number of siblings, country of birth, and parents' country of origin. We also collected MOE data on dates of birth for the population of 4th-6th graders, beyond those who participated in the math and language GEMS.

The school-level data used in this study are derived from MOE records reporting enrollment, school sector (religious etc), and a school's index of socioeconomic status (SES). We obtained two enrollment variables: November enrollment, reported by school headmasters to the MOE, and a June enrollment variables computed by summing spring class sizes. The November and birthday based imputation generate our instruments, while the June data provide the endogenous class size variable that gets instrumented.

As described in the main text, we construct enrollment figures of how many students should be in a class based on student birthdays. Appendix Figure A8 extrapolates Chanukah-based school entry into fifth-grade, showing the grade enrollment determined by application of this rule to birth dates 11 years earlier. For example, students born between December 18, 1990 (the last day of Chanukah in 1990) and December 8, 1991 (the last day of Chanukah in 1991) should have been enrolled in first grade in the school year ending Spring 1998 and, assuming no grade repetition or skipping, been seen in 5th grade in the school year ending spring 2002. Students born after December 8, 1991 should have had to wait for first grade until the school year ending in 1999 and therefore been in 5th grade one year later, in the school year ending spring 2003.

The 1991 and 1992 samples are those used by Angrist and Lavy. The 1991 data are posted at <https://economics.mit.edu/faculty/angrist/data1/data/anglavy99>. These are class-level averages.

# Appendix Tables and Figures

Table A1: Descriptive Statistics (2002-11 5th Graders)

Variable	Mean	S.D.	Quantiles				
			0.10	0.25	0.50	0.75	0.90
Panel A. Class Level Data							
June class size	28.1	5.85	21	24	28	32	35
Number Tested in Language	25.5	6.52	18	22	26	30	33
Number Tested in Math	25.7	6.21	18	22	26	30	33
Language score	71.7	8.12	61.5	67.0	72.5	77.3	81.2
Math score	67.6	10.7	53.7	61.6	68.7	75.3	79.9
Number of classes	8,823						
Panel B. School Level Data							
June enrollment	58.4	27.1	26	40	55	76	95
November enrollment	59.0	27.3	25	41	55	77	96
Birthday-based enrollment	63.8	28.7	29	43	60	82	103
SES index	5.16	2.44	2.02	3.11	5	7	9
Number of classes	2.08	0.78	1	2	2	3	3
Religious school	0.31	0.46	0	0	0	1	1
Number of schools	4,245						
Panel C. Student Level Data							
Language score [N=225,108]	72.1	17.4	48.4	63.4	75.6	84.7	91.0
Math score [N=226,832]	68.1	20.6	37.9	55.3	72.3	84.3	91.5
Father's years of education	11.8	5.03	0	11	12	15	17
Mother's years of education	12.1	4.67	7	12	12	15	17
Number of siblings	1.75	1.24	0	1	2	2	3
Boy	0.50	0.50	0	0	1	1	1
Native	0.90	0.31	0	1	1	1	1
Israeli ethnicity	0.58	0.49	0	0	1	1	1
Ethiopian ethnicity	0.03	0.18	0	0	0	0	0
Former USSR ethnicity	0.16	0.36	0	0	0	0	1
Asia-Africa ethnicity	0.12	0.32	0	0	0	0	1
Europe-America ethnicity	0.11	0.31	0	0	0	0	1
Number of students	240,310						

Notes: This table reports descriptive statistics for the sample of fifth grade students in Jewish state elementary schools who participated in the GEMS tests in math and/or language in 2002-2011. Means and standard deviations for class level data are computed using one observation per class; Means and standard deviations for school level data are computed using one observation per school; Means and standard deviations for student level data are computed using one observation per student.

Table A2: First Stage Estimates Using November Enrollment Instruments (2002-2011)

	Language			Math		
	(1)	(2)	(3)	(4)	(5)	(6)
$f_{jt}$	0.6247 (0.0186)	0.6115 (0.0189)	0.6183 (0.0186)	0.6237 (0.0186)	0.6108 (0.0189)	0.6175 (0.0186)
SES index	-0.0869 (0.0362)	-0.0769 (0.0360)	-0.0815 (0.0361)	-0.0860 (0.0362)	-0.0762 (0.0360)	-0.0809 (0.0361)
November enrollment	0.0268 (0.0031)	0.0726 (0.0093)		0.0272 (0.0030)	0.0722 (0.0093)	
Enrollment squared/100		-0.0275 (0.0055)			-0.0271 (0.0055)	
Piecewise linear trend			0.0618 (0.0061)			0.0623 (0.0061)
$R^2$	0.519	0.523	0.521	0.520	0.523	0.521
$F_{st}$	1124.6	1043.2	1105.7	1118.9	1039.1	1101.9
$N$	225,108			226,832		

Notes: This table reports first-stage estimates for the 2SLS estimates reported in Table 1 in the text. Maimonides Rule  $f_{jt}$  is computed using November enrollment. See the notes to Table 1 for additional covariates in these models. Standard errors reported in parentheses are clustered at the school and year level.

Table A3: First Stage Estimates Using Birthday-based Imputed Enrollment (2002-2011)

	Language			Math		
	(1)	(2)	(3)	(4)	(5)	(6)
$f_{jt}$	0.3257 (0.0178)	0.3037 (0.0181)	0.3060 (0.0180)	0.3256 (0.0177)	0.3039 (0.0180)	0.3061 (0.0179)
SES index	-0.1301 (0.0449)	-0.1100 (0.0445)	-0.1123 (0.0445)	-0.1238 (0.0450)	-0.1041 (0.0446)	-0.1066 (0.0447)
Birthday-based enrollment	0.0547 (0.0031)	0.1355 (0.0110)		0.0551 (0.0030)	0.1345 (0.0109)	
Enrollment squared/100		-0.04580 (0.0062)			-0.0451 (0.0062)	
Piecewise linear trend			0.1384 (0.0066)			0.1390 (0.0065)
$R^2$	0.344	0.355	0.355	0.346	0.356	0.357
$F_{st}$	336.1	281.3	289.3	338.3	283.4	291.6
$N$	225,108			226,832		

Notes: This table reports first-stage estimates for the 2SLS estimates reported in Table 2 in the text. Maimonides Rule  $f_{jt}$  is computed using birthday-based enrollment. See the notes to Table 2 for additional covariates in these models. Standard errors reported in parentheses are clustered at the school and year level.

Table A4: Replication of 1991 Results

	Language			Math		
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A. 5th Grade Data</b>						
Class Size	-0.2772 (0.0758)	-0.2631 (0.0937)	-0.1899 (0.122)	-0.2311 (0.0985)	-0.2644 (0.123)	-0.2053 (0.1450)
November Enrollment	0.0223 (0.0091)	0.0131 (0.0262)		0.0410 (0.0117)	0.0631 (0.0355)	
Enrollment Squared/100		0.0042 (0.0100)			-0.0100 (0.0138)	
Piecewise Linear Trend			0.1369 (0.0359)			0.1940 (0.0430)
<i>N</i>	2,019	2,019	1,961	2,018	2,018	1,960
<b>Panel B. 4th Grade Data</b>						
Class Size	-0.1329 (0.0608)	-0.0739 (0.0683)	-0.1472 (0.0887)	-0.0497 (0.0747)	-0.0328 (0.0845)	-0.0982 (0.0990)
November Enrollment	0.0046 (0.0079)	-0.0396 (0.0218)		0.0198 (0.0093)	0.0072 (0.0274)	
Enrollment Squared/100		0.0210 (0.0095)			0.0060 (0.0124)	
Piecewise Linear Trend			0.1001 (0.0260)			0.1301 (0.0290)
<i>N</i>	2,049	2,049	2,001	2,049	2,049	2,001

Notes: This table reports 2SLS estimates using 1991 data and the specifications reported in Angrist and Lavy (1999). Standard errors are clustered by school. The piecewise linear control specification omits schools with enrollment over 160. Estimates in both panels are from models including controls for a school-level index of socioeconomic status.



Table A5: Maimonides Rule Effects on Socioeconomic Status (2002-2011)

	Using November Enrollment			Using Birthday-Based Enrollment		
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A. Without Additional Controls</b>						
$f_{jt}$	-0.0224 (0.0083)	-0.0195 (0.0086)	-0.0198 (0.0086)	-0.0090 (0.0082)	-0.0063 (0.0083)	-0.0053 (0.0083)
Enrollment	-0.0218 (0.0022)	-0.0305 (0.0067)		-0.0217 (0.0020)	-0.0307 (0.0066)	
Enrollment squared/100		0.0060 (0.0041)			0.0058 (0.0038)	
Piecewise linear trend			-0.0407 (0.0043)			-0.0437 (0.0042)
$R^2$	0.162	0.163	0.161	0.157	0.158	0.156
<b>Panel B. With Additional Controls</b>						
$f_{jt}$	-0.0066 (0.0073)	-0.0066 (0.0075)	-0.0066 (0.0075)	0.0024 (0.0074)	0.0024 (0.0075)	0.0034 (0.0075)
Enrollment	-0.0149 (0.0020)	-0.0148 (0.0062)		-0.0144 (0.0019)	-0.0144 (0.0061)	
Enrollment squared/100		-0.00007 (0.0039)			0.00002 (0.0036)	
Piecewise linear trend			-0.0266 (0.0040)			-0.0278 (0.0040)
$R^2$	0.320	0.320	0.318	0.317	0.317	0.315
$N$	4,245			4,245		

Notes: This table reports OLS estimates of the effect of Maimonides Rule on a school-level index of socioeconomic status. Columns 1-3 report estimates using November enrollment; columns 4-6 report estimates using birthday-based imputed enrollment. Estimates in Panel A are from models including controls for religious school dummy and year fixed effects. Estimates in Panel B are from models adding school averages of father's years of schooling and number of siblings. Standard errors reported in parentheses are clustered at the school level.

Table A6: 2SLS Donuts Using 1991 Data

	Language		Math	
	(1)	(2)	(3)	(4)
<b>Panel A. 5th Grade</b>				
Donut:				
[39, 41]	-0.2340 (0.0762)	-0.2010 (0.0954)	-0.1947 (0.1019)	-0.2144 (0.1308)
[38, 42]	-0.2406 (0.0776)	-0.2072 (0.0987)	-0.2000 (0.1044)	-0.2213 (0.1370)
[37, 43]	-0.2152 (0.0777)	-0.1696 (0.0991)	-0.1930 (0.1055)	-0.2024 (0.1390)
<b>Panel B. 4th Grade</b>				
Donut:				
[39, 41]	-0.1267 (0.0612)	-0.0581 (0.0690)	-0.0544 (0.0749)	-0.0353 (0.0858)
[38, 42]	-0.1187 (0.0632)	-0.0431 (0.0719)	-0.0438 (0.0775)	-0.0208 (0.0899)
[37, 43]	-0.1166 (0.0649)	-0.0390 (0.0743)	-0.0467 (0.0794)	-0.0227 (0.0927)
Controls:				
Percent Disadvantaged	X	X	X	X
Enrollment	X	X	X	X
Enrollment Squared /100		X		X

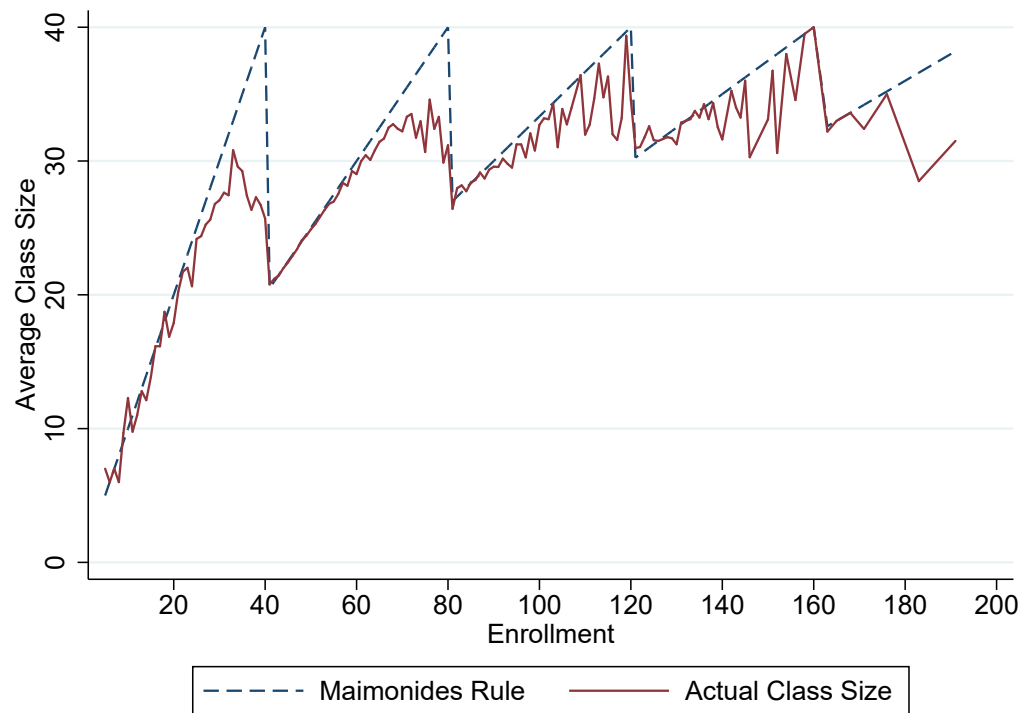
Notes: This table reports 2SLS estimates of class size effects omitting data in the intervals indicated, using the 1991 data analyzed by Angrist and Lavy (1999). Standard errors are clustered by school.

Table A7: Class Size Effects Estimated in a Sample Omitting 2004-6 Data

	Language				Math			
	OLS	2SLS	2SLS	2SLS	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>Panel A. November Enrollment</b>								
Class size	0.0261 (0.0226)	0.0064 (0.0408)	-0.0120 (0.0431)	-0.0079 (0.0419)	0.0424 (0.0311)	0.0103 (0.0586)	-0.0071 (0.0608)	-0.0085 (0.0597)
SES index	-0.6261 (0.1600)	-0.6285 (0.1600)	-0.6250 (0.1688)	-0.6211 (0.1601)	-1.0915 (0.2652)	-1.0949 (0.2653)	-1.0918 (0.2655)	-1.085 (0.2655)
November enrollment	-0.0013 (0.0047)	0.0004 (0.0056)	0.0361 (0.0192)		-0.0033 (0.0066)	-0.0005 (0.0081)	0.0336 (0.0265)	
Enrollment squared/100			-0.0210 (0.0107)				-0.0201 (0.0148)	
Piecewise linear trend				0.0087 (0.0124)				0.0090 (0.0179)
<b>Panel B. Birthday-based Imputed Enrollment</b>								
Class size	0.0239 (0.0225)	-0.0248 (0.0823)	-0.0580 (0.0925)	-0.0484 (0.0901)	0.0294 (0.0312)	-0.0603 (0.1173)	-0.0875 (0.1309)	-0.0905 (0.1282)
SES index	-0.6233 (0.1600)	-0.6300 (0.1600)	-0.6255 (0.1690)	-0.6246 (0.1601)	-1.0746 (0.2654)	-1.0844 (0.2663)	-1.0808 (0.2667)	-1.0781 (0.2667)
Birthday-based enrollment	-0.0003 (0.0045)	0.0037 (0.0083)	0.0409 (0.0260)		0.0022 (0.0062)	0.0097 (0.0116)	0.0400 (0.0353)	
Enrollment squared/100			-0.0198 (0.0118)				-0.0162 (0.0157)	
Piecewise linear trend				0.0174 (0.0212)				0.0333 (0.0299)
<i>N</i>	136,877				137,749			

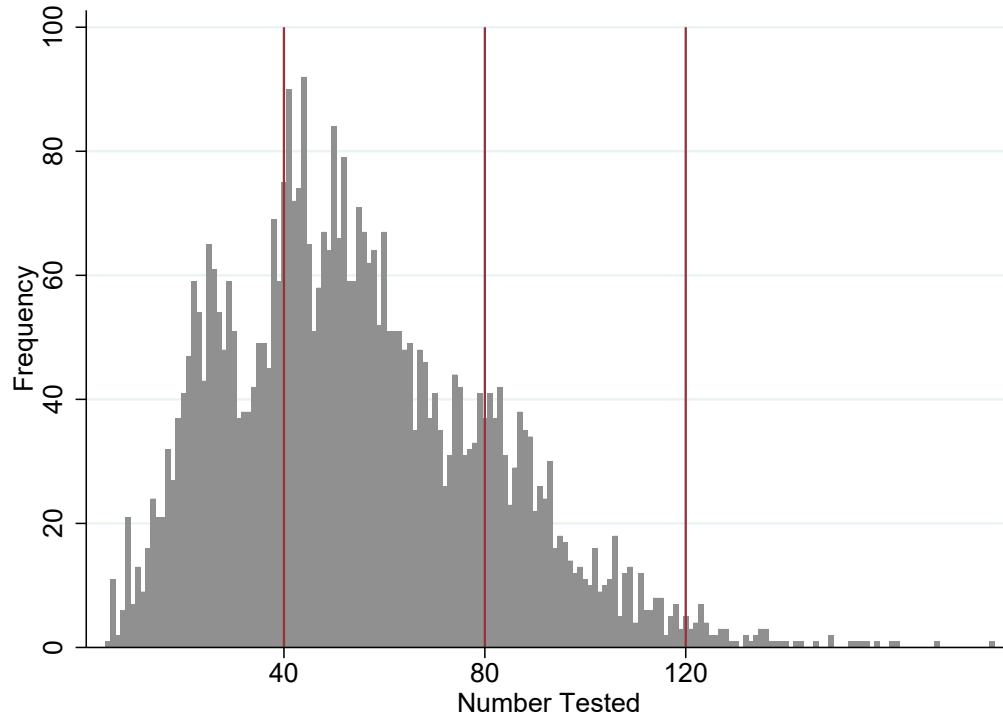
Notes: This table reports OLS and 2SLS estimates of equation (3), omitting the years 2004-2006. The endogenous variable is June class size; estimates in panel A use November enrollment and estimates in Panel B use birthday-based imputed enrollment. Standard errors reported in parentheses are clustered at the school and year level. Covariates are as in Tables 1 and 2 except the reported SES coefficient which is for 2007 only.

Figure A1: June Class Size in 2002-2011, for 5th Graders, Conditional on November Enrollment



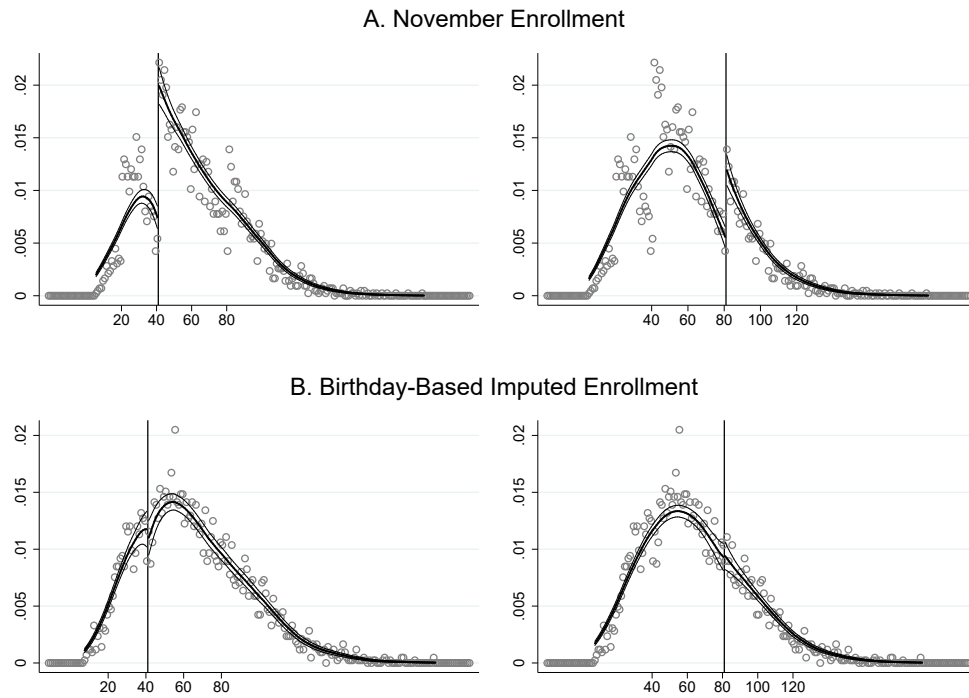
Notes: This figure plots unweighted average (June) class size and Maimonides Rule forecasts conditional on November enrollment in a sample containing averages for each school and year. The underlying data include an average of 563 schools each year between 2002-2006 and 286 schools each year between 2007-2011.

Figure A2: The Enrollment Distribution for Tested Fifth Graders (2002-2011)



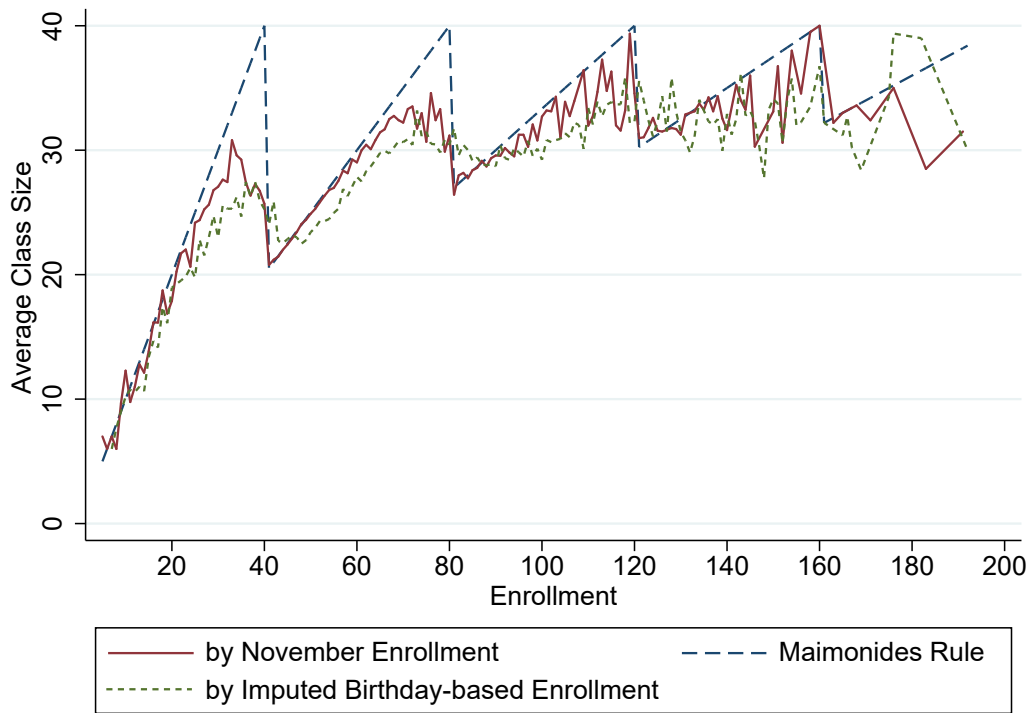
Notes: This figure plots the distribution of the number of 5th graders tested (given by the larger of the number tested in math and the number tested in language).

Figure A3: Density Discontinuity Tests (2002-2011)



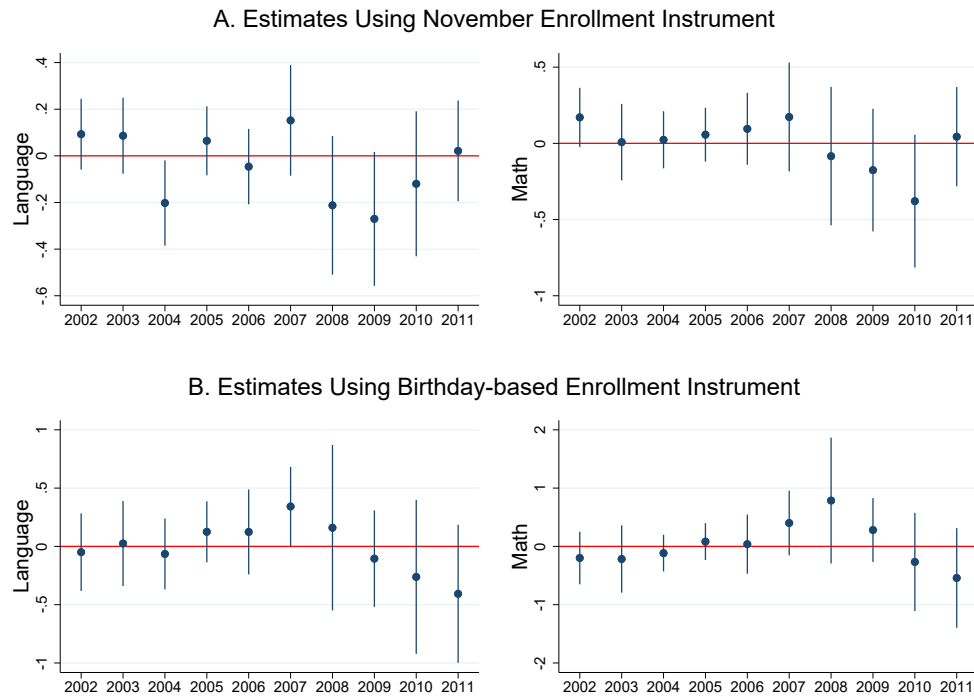
Notes: This figure plots empirical and fitted densities of November enrollment (Panel A) and birthday-based imputed enrollment (Panel B). The figure allows for discontinuities at 41 (left) and 81 (right), with a bin size of 1. Bandwidth, standard errors, and the density plot were produced using McCrary's DCdensity package, following McCrary (2008).

Figure A4: Average June Class Size in 2002-2011  
(Conditional on November Enrollment and Birthday-based Imputed Enrollment)



Notes: This figure plots Maimonides Rule predictions (in blue dash line) and unweighted average June class size, conditional on November enrollment (in red solid line) and conditional on birthday-based imputed enrollment (in green short-dash line), in a sample containing averages for each school and year. The underlying data include an average of 563 schools each year between 2002-2006 and 286 schools each year between 2007-2011.

Figure A5: Estimates of Class Size Effects by Year (2002-2011)

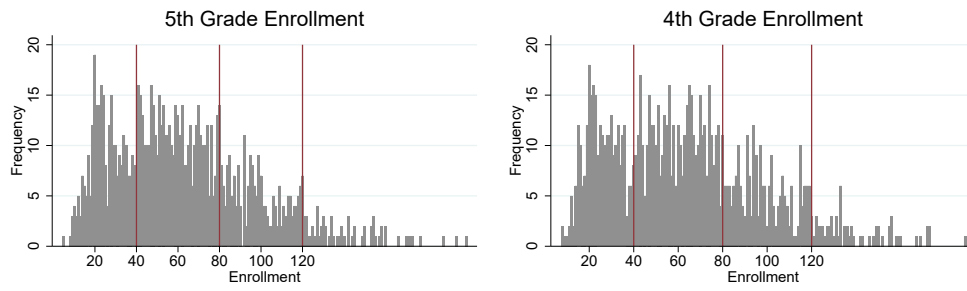


Notes: The figure plots 2SLS estimates and 95 percent confidence intervals for class size effects estimated by year, from 2002 to 2011. The endogenous variable is June class size. Panel A reports estimates using November enrollment. These estimates come from models analogous to those used to construct the estimates in columns (2) and (6) of Table 1. Panel B reports estimates using birthday-based enrollment. These estimates come from models analogous to those used to construct the estimates in columns (2) and (6) of Table 2. Standard errors are clustered at the school and year level.

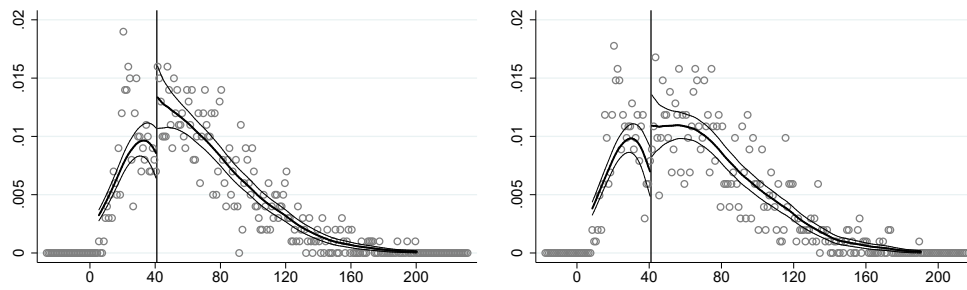


Figure A6: Density Discontinuity Tests in 1991 Data

A. Histograms

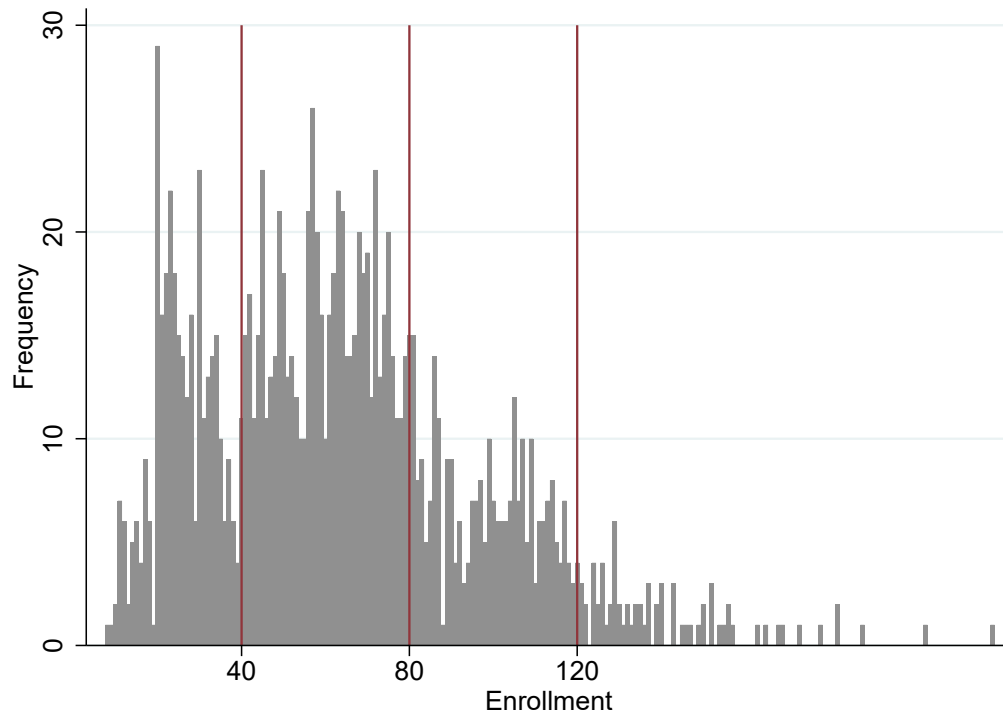


B. McCrary Tests



Notes: Panel A plots the distribution of 4th and 5th grade enrollment as reported by school headmasters in November 1990, for the 1990-91 school year. Reference lines indicate Maimonides Rule cutoffs at which an additional class is added. Panel B plots the densities underlying McCrary (2008) tests for discontinuities at 41 with bin size 1, estimated using the same data.

Figure A7: The Enrollment Distribution for Third Graders (1992)



Notes: This figure plots the distribution of 3rd grade enrollment as reported by school headmasters in November 1991, for the 1991-92 school year. Reference lines indicate Maimonides Rule cutoffs at which an additional class is added.

Figure A8: The Enrollment Distribution for Third Graders (1992)

		Month and Day of Birth																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																				
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Notes: This sketch shows birthday cutoffs for school entry, and their implications for 5th grade enrollment in a pure birthday-based world with no grade repetition or skipping. For example, students born between Dec 18 1990 and December 7 1991 should be enrolled in 5th grade in Spring 2002. Our predicted enrollment variables for 2002-2011 applies these rules to the birthday distribution of children observed enrolled in 4-6th grade in Spring of each year.